

# ***Interactive comment on “Soil greenhouse gases emissions reduce the benefit of mangrove plant to mitigating atmospheric warming effect” by Guangcheng Chen et al.***

## **Anonymous Referee #1**

Received and published: 21 March 2016

This paper describes greenhouse gas emissions (CO<sub>2</sub>, N<sub>2</sub>O and CH<sub>4</sub>) from three mangrove forests in China. The authors provide GHG emissions over each of four seasons. These are useful measurements to add to the growing number of studies of GHG emissions from coastal wetlands. The authors could improve the context of the paper. Why were the three sites that were studied chosen? The IPCC Wetland supplement (2013) is an important contextual document that could be cited. The Supplement suggests that CH<sub>4</sub> emissions occur with salinity < 15 ppt, as seems to be the case in this study. There are recent papers on GHG emissions not cited by the authors including the work by Maher et al. The hypotheses could be improved. In the paper the relationship between GHG emissions and soil parameters are reported (as correlation

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coefficients) but there are no explicit hypotheses stated. Table 3 is the center piece of the study, yet there is a lot of information missing of how the authors arrived at these values. The authors use measures of NPP derived from litterfall ( $2.75 \times$ ) and an annual value of soil respiration to calculate Net Ecosystem Production (NPP - SR). An annual plant CO<sub>2</sub> sequestration rate is calculated as  $NPP \times 44/12$  and then compared against the annualized GHG emissions to provide an Ecosystem mitigation potential; and the GHG emissions are assessed as a proportion of annual NPP, although the % values are not provided in Table 3. The authors need to explicitly indicate in the methods how they scaled up their point measurements to annual values. How was tidal variation and seasonal variation incorporated into the scaling up? The calculations do not include C inputs due to allochthonous C sources trapped in sediments which may be large at this site? In the Methods section the authors claim that all CO<sub>2</sub> fluxes from the soils are derived from heterotrophic metabolism as chambers were not deployed over above-ground roots. But below ground roots are dense in mangrove forests and thus the claim needs to be better substantiated. The citation to Tomlinson is not appropriate as this is a botanic text with no reference to gas fluxes from heterotrophic vs. autotrophic sources. There are lapses in English expression that need to be corrected. Providing the dry bulk density of sediments would also be useful. Table 3 is confusing. More information needs to be provided in the caption.

Smaller points Title: mangrove plants (plural) or mangrove forests L17 – plural for plants. Forests maybe more suitable word L16 – what direction of gas fluxes? Add a statement about the direction (uptake; losses?) L20 – is 22% a large proportion? The authors are overstating the case. A large proportion would be most of the GHG gains being lost because of simultaneous methane emissions. L26 – remove word “problems” L28 – The CO<sub>2</sub>. . . P2L9 – and as detritus in the sediment P2L11 rewrite P2L12 – alternating P2L22 place the citations at the end of sentences P2L26 Alongi indicates lower proportional loss of CO<sub>2</sub> due to soil respiration. However, losses through tidal exchange may also be high  $\sim 30\%$  (Maher et al.; Boullion; Alongi) P3L 30 – high levels of spatial variation P4first paragraph. Needs some English editing for clarity P4L18

- inserting chambers 3 cm. Would this result in severed roots? P6 throughout the results P values are listed as 0.000 – this is not correct for the reported F statistic. P7L1 Relationships between GHG fluxes and soil characteristics need to be shown (plotted) rather than just present Pearson correlation coefficients. Pearson CC can be very misleading when data is not normally distributed. P7L11 Litterfall production P8L8 Is 22% 'large'? I think 22% as a large proportion is overstating. How does this compare with values provided in the IPCC Supplement? P9L19 Does this suggest an autotrophic source for soil respiration? P9L29 – 'unelectable' this is not the correct word to use here P9L33-34 a statement provided without a reference. This is a part of the need for the authors to more clearly articulate the scaling up approach in the methods section. P10L1-6 this discussion is related to assumptions made when scaling up measurements. But the approach is not described. P10L20 stored in biomass and as detritus? P10L23 omit "in the mangrove wetland" P11L5-9 English needs attention P11L11 – largely offset is overstating for a 22% offset. Partially offset would be more appropriate. Table 1 Reproduction (remove hyphen) Table 2 – caption needs to include abbreviations e.g. OC, TKN Fig 1 – could this be improved to add more information? It locates us in the estuary, but does not do much else.

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Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2015-662, 2016.

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